

October 15, 1941.

Dr. F. M. Burnet
Walter & Eliza Hall Institute of Research
Melbourne, Australia

Dear Doctor Burnet:

I want to thank you most heartily for sending me your beautiful monograph on the "Production of Antibodies". It arrived today and I have already read it from cover to cover with absorbed interest. It seems to me that you have picked out the soundest part of Bergmann and Niemann's work on which to base your theory - the analytical part and calculations in their work will certainly have to be revised. I have never taken the Breinl-Haurowitz-Mudd-Pauling theory as literally as the various authors' similes and diagrams might lead the reader to suspect they did, and for this reason I think your protease-modification concept is in general harmony with the underlying ideas of the older theory, and as you have shown, is a more reasonable expression of present knowledge. I am glad, too, that your theory and Dr. Sabin's seem essentially different aspects of the same mechanism. If you have not already sent her a copy I shall show her mine when she comes East this winter. I am sure she will be greatly pleased.

I was also much interested in the slow and lasting antibody response to the Cl6 phage and your other data on primary and secondary responses. Do you think, however, that a logarithmic increase of "titer" necessarily means an equally great increase in antibody concentration? The mere fact that 25 to 35 per cent of cross-reacting anti-Ea or anti-S VIII can be removed with no appreciable loss in "titer" suggests that actual antibody content may change quite differently. And again, apropos of p.68, is it not probable that pneumococci are as "good" antigens as Salmonella, for if 1 mg. of anti-S I, II, or III nitrogen per ml. corresponds roughly to an agglutination titer of 1 to 1000, an anti-O or -H titer of 1:100 000 cannot mean 100 mg. of antibody nitrogen per ml., since sera do not contain even that much protein. Rather, the combining proportions of antigen and antibody in the two systems are so different that comparison of "titers" leads to impossible results. I bring up these very minor points because they are close to experiments we have done.

I am very glad that some of the work of this laboratory has been of help in the evolution of your ideas on antibodies. As a

poor return for your monograph, I am sending under separate cover a few recent reprints, including several on the extension of the quantitative method and theory to complement and its behavior. We also have some experiments under way which will eventually, I hope, throw light on the peculiar antibody response of the ungulates.

Again with many thanks and all good wishes,

Sincerely yours,

MH/m

Michael Heidelberger.